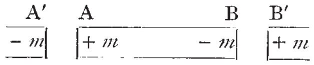
Mr. Shelford Bidwell (*Phil. Trans.*, 1888, pp. 216–7). He has been followed by Prof. Ewing, on pp. 242–3 of his "Magnetic Induction."

On the other hand, Maxwell, *l.c.*, speaks of a *tension* along the lines of force, and even more explicit statements to the same effect are due to Kirchhoff (*Wied. Ann.*, Bd. 24, p. 52, and Bd. 25, p. 601) and Prof. J. J. Thomson ("Applications of Dynamics to Physics and Chemistry," p. 52). The latter says: ". . . there are the strains arising from Maxwell's distribution of stress. Kirchhoff has investigated the effect of this on a small soft iron sphere placed in a uniform magnetic field and has shown that it would produce an *elongation* . . . along the lines of force and a contraction at right angles to them. We may therefore assume that in general this distribution of stress causes an *expansion* . . . in the direction of the lines of force . . ." (italics mine).

Prof. Thomson's view was accepted and restated by myself in discussing the phenomena in cobalt (*Phil. Trans.*, 1890, p. 339). The fact that my views escaped Dr. More's notice is not surprising, but it is strange that he overlooked Prof. Thomson's statement, and that I overlooked Mr. Shelford Bidwell's. My belief now, as before, is that the view of Kirchhoff and J. J. Thomson is the correct one.

Let us take the case of a long iron bar magnet suspended vertically. Suppose it cut in two transversely at a section A; then, supposing the magnetism sufficiently intense, the lower portion will, we know, adhere to the upper until a considerable weight, W, is hung from the lower end, B. If *w* be the weight of AB, the magnetic force over A must balance W + *w*, acting downwards, and so must itself act upwards, or away from AB. In other words, the magnetic force across A acts precisely like the elastic stress across a section of a rope fastened at one end and pulled at the other. The rope, as every one will realise, is under tension not pressure, and so clearly is the iron bar.

A more general way of looking at the matter is as follows:



Take imaginary sections A and B of a long uniformly magnetised straight or bent bar, and regard coterminous surfaces AA′ and BB′ as separated by distances small compared to AB. Taking the usual elementary theory, we see the force acting on the distribution $+m$ over A is F − F′ + F″ to the *left*, where F arises from $-m$ over A′, −F′ from $-m$ over B, and F″ from $+m$ over B′. Now clearly F′ and F″ are approximately equal, and each is small compared to F, so that A is obviously pulled to the left. Similarly B is pulled to the right, or AB is under tension.

Of course if the bar, when supported horizontally on a frictionless table, were actually cut through at A, foreign material—a finger, for instance—inserted between the naturally coterminous surfaces A and A′ would experience compression. I cannot avoid the conclusion that Mr. Shelford Bidwell and Dr. More have fallen into confusion through regarding the problem from this point of view, forgetting that the surfaces supposed to attract one another have nothing between them to squeeze.

That the question is of vital importance to a true understanding of the phenomena in iron is unquestionable if Dr. More's experiments—which seem carefully designed—gave anything like the true results. A reversal of the sign in his correction $B^2\delta l/8\pi l$ in his tables I. to IV.—necessitated by the view I advocate—would completely alter the character of the physical deductions to be made. The occasion for the correction, I may add, is as clearly indicated by Mr. Shelford Bidwell as by Dr. More, but the experimental data obtained by the former led him to attach less importance to it.

It is not my intention to discuss the actual magnitude of the magnetic stress or of the consequent alteration in length. An interesting experimental paper on the former subject by Mr. E. Taylor Jones, with references to recent conflicting authorities, appeared in the *Phil. Mag.*, March 1895, pp. 254–267. I may say, however, that the theoretical importance of investigations such as those of Prof. J. J. Thomson into the mutual relationships of magnetism and elasticity renders it all the more desirable that any doubt as to the true nature of the experimental results should be removed. To be altogether satisfactory, these investigations require trustworthy magnetic and elastic data for one and the same specimen. C. CHREE.

January 11.

The Metric System.

MR. BROOK-FOX does not state accurately, in his letter to NATURE of January 9, what passed in relation to legislation on the subject of Weights and Measures in British India in the years 1870–1871.

It is true that an Act was passed in April 1870, enabling the Governor-General in Council to require the use of the Metrical Weights and Measures by Government departments, railway or other companies, registered under the law relating to such companies, and persons exercising specified occupations or trades; but only after certain preliminary steps had been gone through.

This Act was disallowed by the then Secretary of State for India, the Duke of Argyll, who had at first authorised legislation on the subject, in the general direction given to this Act, but considered that the actual form in which the measure was passed went beyond his original instructions, as it might have been extended to the dealings of persons engaged in business or trade, and was otherwise considered to be too drastic. Objection was also taken to the inclusion of the metre as the standard of length, the original authority to legislate having been limited to the adoption of the kilogramme as the unit of weight.

Immediately on the receipt of the Secretary of State's disallowance of the Act of 1870, a new permissive Bill was prepared, extending only to the standard of weight, which was again stated to be the kilogramme, and of measures of capacity which are subsidiary to those of weight; and limited, as to the power of applying it, to the case of Government departments, municipal bodies, and railway companies.

This measure became law as Act xxxi. of 1871, and is still in force. But it has never been put into operation. This, however, was not the result of the Duke of Argyll's action, but of the change of policy that followed on Lord Mayo's most unfortunate murder. Had Lord Mayo lived, he would certainly have given effect to the Act, the importance of which he thoroughly recognised. His successor was of a different opinion, and the combination of circumstances that for the moment appeared to have rendered possible the introduction of a rational system of weights and measures into our Indian Empire, passed away, and has not yet returned.

As having had charge of the Bill of 1870, in the Council of the Governor-General, I may be permitted, in conclusion, to quote the words that I wrote twenty-five years ago with reference to the fears expressed by the Secretary of State as to the danger of precipitate action in such a matter as this.

"The history of this very Act regarding weights and measures, which has been under consideration in one form or other for not less than fourteen years, which in its last stage has taken seven or eight years in coming to maturity, and to carry out the provisions of which no measures have yet been taken, or can be taken in all probability for another year at least, seems to me to teach that the one thing perfectly certain in the future is that the progress made in giving effect to any change in the present case will be slow in a degree most painful to all who are concerned in it; and that what with active opposition, and what with indifference, this Act is far more likely to remain a dead letter on the statute book for an indefinite period, than to be brought into operation with improper haste."

January 13. RICHARD STRACHEY.

Marsupial with an Allantoic Placenta.

I HAVE just received from my friend and former pupil Mr. J. P. Hill, Demonstrator of Biology in the Sydney University, an advanced copy of the *Abstract of Proceedings* of the Linnean Society of New South Wales for November 27, in which he announces the discovery of a true allantoic and highly vascular placenta, of a discoidal and most probably deciduous type, in the Bandicoot (*Perameles obesula*).

In the second part of his magnificent "Forschungsreisen im Australien und dem malayischen Archipel," Prof. R. Semon has substantiated the discovery of apposition between the allantois and sub-zonal membrane in *Phascolarctus*, and shown that a fusion is effected. He points out that the fœtal appendages of that animal are of a type involving certain Marsupialia in a position intermediate between the Placentalia on one hand, and the Monotremata and Sauropsida on the other, and regards the earlier recognised Marsupial condition, in which the allantois is remote from the serous membrane, as secondary and associated with the loss of a respiratory function by that organ.

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The condition described by Mr. Hill in *Perameles* is one of natural advance upon the Phascolarctus type, and the facts in proof of the intimacy of relationship between the Placentalia and Lower Mammalia now overlap, like those bearing on birds and reptiles—*i.e.* just as *Archæopteryx* may be regarded as an avian reptile, and the Odontorinthes as reptilian birds—so the *Phascolarctus* may be regarded as a Placental Marsupial, and those placentalia which develop a provisionally vascular yolk-sac extending to the serous-membrane as Marsupial if not Monotrematous Placentalia. Mr. Hill's discovery, coming close upon that of Woodward that the young of the Diprotodontia are found to develop at fewest five pairs of upper incisors, and of Thomas that a representative of the Epanorthidæ survives in South America, and at a time when the tooth-genesis of both Marsupialia and Placentalia is receiving exceptional attention, is as welcome as important, in assisting us to form a clearer conception of the inter-relationships between these mammals.

Mr. Hill's observation would appear to lend not a little support to the conclusion which has for years been steadily gaining ground (*cf.* Nature, vol. xl. p. 420), that the allantoic placenta was primitively discoidal. In an accompanying letter, he informs me that he has more recently come into possession of a uterus containing an unattached blastodermic vesicle; and it is sincerely to be hoped that he will be able to furnish observations bearing directly upon the important question of the supposed primitively chorionic nature of the Mammalian placenta.

G. B. Howes.

Royal College of Science, London, January 14.

The Origin of Plant Structures.

May I call attention to a serious omission, and reply to one or two points, in Mr. Barber's review of my book? He says: "Of the *inheritance* of such acquired characters there is no proof at all. We are offered instead the 'argument of coincidences' and the 'cumulative evidence of probabilities, which amounts to a moral conviction.' Clearly, before rejecting a well-established and widely applicable hypothesis, something more tangible is required" (Nature, December 19, 1895, p. 145).

First, with regard to inheritance. Mr. Wallace also asked for some proof of this; and I reply again that nature herself supplies it; for plant structures *are* reproduced by seed every year. It is the previous question, "How have they arisen?" with which I am concerned. But I *have*, in fact, given plenty of cases: as in my experiment with *Ononis*, Buckman's parsnip, Flahoult's with alpine plants, &c.

Moreover, the objector should state whether he means that any altered features in a plant should be reproduced by seed irrespective of the environment, or not. If a plant changes under new conditions—as ample experience shows it may—of course all its offspring will follow suit, under the same environment, irrespective of heredity; and if the conditions be maintained long enough, *then* the new features will tend to become relatively fixed, as all cultivators know. As long, however, as a natural environment is constant, no varieties are, as a rule, to be expected. Under cultivation, this rule does not hold good. Thus, *e.g. Brassica oleracea* gives rise to no varieties in nature; but there are very many fixed and hereditary races in artificial soils.

Secondly, the truth of an hypothesis or deduction cannot be more surely established than by "verification by experiment." Thus, with desert, aquatic, alpine, maritime, and other plants, I not only established the truth of my contention by *induction*, but have given the experimental verifications both of others and myself. For *this* fact Mr. Barber gives me no credit. It is in these two lines of proof, viz. by *induction* and *experiment*, that the theory of "the origin of species by means of natural selection" is wanting. It is, as Mr. Barber says, based on an "assumption," and is an *à priori* deduction that, because plants can vary indefinitely under cultivation, therefore they do so also in nature. *This has never been verified.*

Mr. Barber adds: "It is usually agreed that, from the nature of the case, a definite proof of the action of natural selection is difficult, if not impossible, in the present state of our knowledge." Is not this a most damaging admission? If the word "present" has to cover the thirty-six years since the "Origin" appeared, it would seem to be about time to abandon the theory even as a working hypothesis. The "wideness of its application" is no test of the truth of a deduction; for though natural selection may account for all organic structures, it can only do so because it is *assumed* that it can account for them. Herein it agrees precisely with the theory of special creations; which is equally assumed to be capable of accounting for every organic structure.

Mr. Barber tells me that I show a want of good taste in "narrowing" Darwin's field of observation. I much regret that anything I have written should be regarded as uncourteous; but it is Darwin himself who admits the "imputation," for he wrote: "I will give in detail all the facts which I have been able to collect," *i.e.* of "definite action" in plants, and mentions about thirty instances which he had heard of. He did not believe in definite variation being the rule in nature.

Lastly, I make no claim "to reconstruct the theory of evolution." All I have done is to take the following passage of Mr. Herbert Spencer's Essay on "The Development Hypothesis" (published in 1852, seven years before Darwin's "Origin" appeared) as my subject; and I have simply verified its profound truth in its application to plants. "The supporters of the developmental hypothesis can show . . . that any existing species—animal or vegetable—when placed under conditions different from its previous ones, immediately begins to undergo certain changes of structure fitting it for the new conditions . . . that in successive generations these changes continue until ultimately the new conditions become the natural ones. . . . They can show that throughout all organic nature there is at work a modifying influence of the kind they assign, as the causes of . . . specific differences; an influence which, though slow in its action, does in time, if the circumstances demand it, produce marked changes."

George Henslow.

I am sorry I cannot agree with Prof. Henslow as to the nature of the proof required of him.

The fact that "plant structures *are* reproduced by seed every year" in nature is surely no proof of the inheritance of acquired characters!

Plants are, it is true, exceedingly plastic structures, and, as all allow, are both temporarily and permanently affected by their surroundings. This, if we needed further proof, Prof. Henslow has repeatedly demonstrated by his interesting series of facts. There seems to be danger, however, of confusing these *changes in the individual* with *changes in the race*. Prof. Henslow makes the former the prelude to the latter; and the first question to be settled is, What connection is there between these two classes of changes? In other words, Are acquired characters hereditary?

The issue is the same if we seek for the *causes of variation*. Darwinism, realising that there is a gradual adaptation of plants to altered surroundings, explains the fact by the *indirect* influence of the environment acting through natural selection. Among plants, which are stimulated to vary in all directions under change of conditions, those are preserved which vary so as to place themselves in adaptation to their new surroundings. Prof. Henslow substitutes the *direct* influence of the environment upon the *individual* plant, and asserts that the changes thus induced "become relatively fixed if the conditions are maintained long enough." Here is an assumption that the *change in the race* is the outcome of the *direct effect of the environment upon the individual*, or, again, that acquired characters are hereditary. I have carefully re-examined the cases mentioned of *Ononis*, parsnips and alpine plants, but cannot trace any proof of this assumption.

As to the *indefinite variation* of plants and animals in nature, it is difficult to conceive of doubt upon the subject. It is a common saying that "no two blades of grass are alike," although conditions could hardly be imagined more uniform than those in one and the same field. Moreover, the fact of indefinite variation has been fully proved by Prof. Wallace in his work on Darwinism. In chapter iii., on "Variability of Species in a State of Nature," the whole subject has been exhaustively dealt with, and I cannot do better than refer Prof. Henslow to that chapter, where numerous cases are given, both in animals and plants. I would especially refer to the extracts from Darwin's note-books there published for the first time.

I do not quite follow Prof. Henslow in rejecting evidence drawn from cultivated plants. Placing wild forms under cultivation is a severe change of environment, and any such change induces, of itself, a great tendency to vary. The influences which work slowly in nature are intensified; and the substitution

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